than any other body excepting hydrogen. E. Lecker has shown to the Vienna Academy that mixtures of methylic alcohol and water have a specific heat higher than that of water, and accordingly take the second place," &c. Can you spare me a corner to point out that in 1869 the specific heats of some mixtures of ethylic alcohol and water were proved by Dr. A. Dupré and myself to be considerably higher than that of water, *e.g.* a mixture containing 20 per cent. alcohol, has a specific heat of 104·3, water = 100 (*Phil. Trans.*, 1869, 591; Watts's Dict., 2nd Supplement, 475). Moreover, we especially mentioned "that our experiments furnished the first example of a liquid having a specific heat higher than that of water." Since 1869 Dr. Dupré has estimated (*Proc. Roy. Soc.*, xx. 336) the specific heats of mixtures of methylic alcohol and water without finding any such mixture to possess a specific heat above that of water.

University College, London F. J. M. PAGE

Age of the Sun in Relation to Evolution

I HAVE read Mr. Plummer's letter along with his article in the *Popular Science Review*, and am surprised at his objections to my paper on the "Age of the Sun's Heat."

It matters not whether the sun's heat was derived from the contraction of his mass or from the falling in of meteorites, or of comets, as advocated by Mr. Plummer in the article referred to, we could only have had a supply of heat sufficient for twenty or thirty million years, at the present rate of radiation. Probably not much more than half of this would be available for the formation of the stratified rocks, and the development of life on the globe; a length of time irreconcilable alike with geology and evolution. We are therefore compelled to look for some other source than gravity as the origin of the sun's heat. It will not do to lengthen this period by assuming that the rate of radiation was less during past ages than at present, because we should have to assume that the temperature in the past was also less, a conclusion at variance with the known facts of geology and of palæontology. I never supposed that the rate of radiation in the past may not have been greater than now. Nor did I ever suppose that there is any antecedent improbability whatever in the collision of stellar masses. What I maintained (*Quart. Journ. of Science* for July, 1877) was that the formation of a sun is an event which, on an average, can only be witnessed once in about 15,000 years, or the number of visible stars would be greater than it actually is. And this, I think, is a sufficient reason why we should not expect any historical record of such an event. Further, it does not necessarily follow that the two bodies coming into collision should possess equal mass or velocity in order to have their motion of translation converted into heat. If it be true that the stars derived their heat from loss of motion then this may quite well explain why their motions are so small. In a word, the energy which the sun has been dissipating into space through past ages, always existed in the form of motion. Collision only changed it from one form to another, viz., from the motion of translation to molecular motion.

JAMES CROLL

The "Phantom" Force[1]

IT might be supposed that permanent and entirely local or "internal" force-pairs of this kind acting on innumerable material couplets in a system would so disturb the individual energies of their motions that no general conclusion as to the total change of energy during the progress of such a system's motion could be drawn; but the simple law that impulses act independently of each other and of existing motions soon shows that the whole gain of energy in the system is the sum of the separate gains in the several mass-couplets due to their absolute or several actions and reactions at every instant of the motion, and that when these abstract force-pairs are all permanent, the above constancy of the sum of their actual and potential energies is possessed by the whole system as perfectly as would be the case by one only of its couplets, or component pairs. That this is not merely an abridged expression for the resulting actual energy in all the possible different phases that such a system may go through, briefly stated for any initial and final configurations and initial motion of the system by means of the negative scale or potential function of all the several component force-pairs supposed known; not merely, that is to say, a logical consequence of arbitrary and fanciful definitions, but a conclusion full of importance and of real natural signification depends, firstly, upon the fact that the thing defined as "impulse," or the gradient of the scale, which is here independent of the time and depends only on the mutual configuration, is not permanent by a very rare occurrence, but that it is often so, and under very various circumstances; and again that this impulse, or flux of momentum, or gradient of energy, occurs in many other motions with conditions of equal simplicity; and lastly, and above all, on the fact pointed out at the beginning of these reflections, that while we are able to use, and of our free will to call into existence force in innumerable ways, we learn from our experience that this impulse is invariably *caused or dictated by* a certain special efficacy or compulsion, which our power of exercising it as we please so as either to annul, to modify, or to increase it at will with the consequence of obtaining with it any effective impulses that we desire, shows us to be a different kind of quantity from the impulses that we either thus obtain, or that we see it producing in surrounding nature. Newton's second law of motion in fact recognises this specific difference between the magnitudes of a force and of its effect, when it asserts that forces *produce their whole effects* (that is to say, remain unaffected in their efficacies) whatever may be the state *of rest* or of motion of the bodies upon which they act. As it is found that forces or compulsions (measured as they are in statics by additions, subtractions, and oppositions to a standard force) are always proportional to the free impulse, or undisturbed acceleration of a mass-unit which they can produce, so that by taking the impulsive effect and the active compulsion of any one standard force as the units for measuring these quantities respectively, they are then said to be numerically equal to each other in every force; it yet follows from their specific independence that they are not identical in kind as they are in measure. The same is true of the products obtained by multiplying them separately by any small space through which a force acts; and it would be an obvious misstatement to assert that the sum of the works of a compulsion, and of the free impulse which it produces taken negatively, is constant when a force acts freely; because this would be confusing in one sum two different quantities; a result which it seems must arise from the simple fact that our part in mechanical "compulsions" distinguishes and removes them from the category of impulses to which they would otherwise belong, and leads us to regard them as *the causes of* the impulses which we observe. The language adopted by Newton (and used also by D'Alembert) in the proposition quoted at the beginning of this letter is that in a proper mechanical system, *compulsions equal to* the observed impulses reversed, will (as is obvious) arrest in their origin all changes of motion in the system, and will (with the immutable force conditions proper to the system) hold in balance, or give a complete account of all the forces (other than those immutable ones) acting upon it. Using the principle of virtual velocities in this case of equilibrium of balancing forces, Newton expresses the rule for exploring all the mechanical efficacies (superadded to the immutable ones) acting in the system by concluding that the sum of all the similar "works of *compulsion*," or of all the "actions" in a short time corresponding to a small motion of the system, when the reversed ones have been introduced, will be a constant quantity. Thus both Newton and D'Alembert agree in this, that they recognise in forces *causes which differ from* the effects which they produce. By what similar laws of work found to hold true in a proper conservative system the modern science of energy (which deals with the phenomena of causation in a wider and more diversified form) seeks to extend the method of cancelling the counteracting causes, or the principle of energy conservation here laid down by Newton for a mechanical one, to the far larger, but less thoroughly explored and exhausted field of all the onward flowing streams of physical agencies which we perceive following their natural bents or inclinations around us, I will presently endeavour to explain. It should be noticed in connection with this general extension of the principle, that the "work" of a force *in a short time*, however fixed its efficacy, or its rate of doing work *in a short space* (or of producing *momentum* in the short time) may be, is incidental, and not a fixed quality of the agent force like its faculty of tension, since a force as often diminishes as increases actual energy by a momentary action, and thus no fixed rule is drawn from the natural tendency of force to impart momentum, that potential energy necessarily becomes, or even necessarily tends to become, actual energy in every mechanical energy-transformation. The mechanical stress of friction is an example of the opposite tendency; and it also furnishes us with an example of a force whose *working power* only, and not its motive tendency, is a mechanical "*agent*" which we can summon up at will; but

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[1] Continued from p. 303.

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of which we still regard the motive tendency as similar to that of other mechanical forces, because it can maintain equilibrium with them.

If the natural office of force as a bond between space and matter is to ward off contact between material points, and to endow them with impenetrability (for this marked feature as far as it has been explored appears to be inherent in all matter) by absorbing at the proper places the energy of motion, and by curbing and accelerating it elsewhere according to these dictates, it must be implanted in material points in such permanent pairs as have just now been described; for the third law in Newton's master-summary is often held (with how much correctness, perhaps, may be questioned), to assert that all the forces of nature consist of an action and a reaction in equal and opposite pairs, and be omnipresent with a particle to protect it, its permanent impulse, or rate of doing work, being at the same time referrible to the space or distance between the mutually impinging, or colliding pair of atoms. A mechanical system so constituted, as we have seen, if not disturbed by the forces of foreign bodies outside of it will have the sum of its actual and potential energies constant. If we include those foreign bodies (endowing their forces at the same time with persistency), and if we find that the whole material universe as far as we can explore it admits of being comprehended in one system of this kind, a mechanical explanation of all known physical agencies might thus, apparently, be arrived at. In every particular of the motion of any group of bodies in it, however, except the single one of its total energy, we would be obliged to abandon (as Newton does) the local centres of reckoning of the several force pairs, and betake ourselves to the mass-centre of the group as our origin of reference for noting all the forces, and tracing all the motions of a body completely in its wanderings through the system. If this obligation, and the end to which it leads us of referring force at last to perfectly abstract realms of space and matter terrifies and affronts the scientific sense, it is sufficient consolation to observe that if force were not ultimately so referred, and if its impulse was exerted in those local spaces only in which we find it acting apparently as a simple action and reaction, there would be as many distinct kinds of energy of motion and of configuration (which we would still retain as expressing the local laws of force) as there are local spaces, with countless complex rules for mutual equivalence of these several energies connected with the common path of the body in several of them together, which would effectually defy even a modern physicist to disentangle and employ! We may rest thankfully contented with the laws that Newton has traced out. But does the simple mechanical system which we have just imagined really represent that of nature? Are nature's force pairs really all permanent? (We will suppose that they are all dual and reciprocal, for, as will presently be noticed, a special and peculiar explanation only can be given of forces which are absolutely external or solitary without any physical qualification); and is the sum of their mechanical energies a constant quantity, as we know that it becomes when all the other kinds of energy in material nature are added to it? The answer is very obvious, but it betrays at the same time a complete ignorance of the extent and depth of the question that we put. The readily-preferred response is "No; the sum of the mechanical energies, as far as they can be recounted, is not constant, for numberless forces, and mechanical energies proceeding from them are being constantly produced by heat, radiation, chemical action, and other physical agencies, or are being employed to renovate those agents with energy in equivalent stores, but whose special kinds *are not* mechanical." At the same time the progress of physical inquiry reveals to us in the operations of these agents microscopical, or rather hyper-microscopical, actions of force, and invisible charges of actual energy, with which our earlier knowledge of these agencies was entirely unacquainted, and a little step of inductive generalisation only is needed (I believe that this view was unfolded by Helmholtz many years ago, but I have not been able to rediscover his remarks and demonstrations[1]) to suppose that all physically-generated forces form but visible and changing resultant-links in an invisible chain of persistent forces, of which we hold some of the most prominent loops in our hands, and thoughtfully wonder what they are.

In his Glasgow lecture on "Force" (in NATURE, vol. xiv. p. 463), an important hint was offered by Prof. Tait regarding an apparent character of potential energy, founded on the mathematical condition that quantities whose units of measurement are of the same dimensions in space, mass, and time, are of the same kind. It follows from this that the potential energy of a force, or the energy received from and transferred to it is of the same kind as actual energy of material motion, because they are both measured by the same combination of the units of space, time, and mass.

Let us first observe that it cannot be *matter* in motion which constitutes the potential energy, unless this matter can traverse itself and other matter freely (because, itself the cause of force, it cannot itself experience any), and therefore that it is something not matter, but both resident in and proportional to matter, and also free; and that its energy of motion as an occupant of matter is actual, and in a state of freedom is potential energy. Imagine a perfect reflection of the material universe to be formed by a plane in space; then the changes of energy of motion of the reflected image of any mass particle taken negatively are equal and opposite to the similar changes of energy of the particle itself, and would measure in a kinetic form the amount of the work of force upon it; but the *tendency* of energy of this form and of the actual form to pass into each other would not be accounted for. In our present knowledge of its transformations it scarcely seems possible that a simpler picture of potential energy as a form of energy of motion than this downright imitation of the actually-existing motions could be reasonably offered. The proposition that force must be a process of transformation of *a new energy of motion*, so astonishingly complex, I confess staggered me, and even led me to doubt if the simple laws of force and motion laid down in the "Principia" could be really so perfect and complete as they appear, amid the pell-mell of motions which the thought suggests! I began this letter shortly before going to Plymouth, intending to recommend much more careful experiments than even Mr. Crookes has carried out with the radiometer, and with his recent, most effective form of the instrument, the otheoscope, in order to test and examine the question of the laws of force (especially with the idea of possibly isolating a single force) seriously; and though much induced to do so by the warm and timely words of commendation passed on Mr. Crookes' labours in his opening address to Section A at Plymouth (NATURE, vol. xvi. p. 314) by Prof. G. C. Foster, I have been unable from other occupations to finish it until now. But I have entirely abandoned my original intention, in great part, by reason of a new light on the exceedingly abstruse and puzzling question which the able remarks by "X" in NATURE (vol. xvi. pp. 438, 457) have afforded me about the real characters of force and of potential energy.

Newcastle-on-Tyne A. S. HERSCHEL

(*To be continued.*)

Cumulative Temperatures

UNDER the above heading, among the "Meteorological Notes" in your issue of last week, I notice the announcement that "To simplify the difficulty of obtaining sums of temperature . . . M. von Sterneck has recently proposed to obtain these indirectly by observation of the sums of actions produced by the temperature." And that M. von Sterneck's proposal is to employ for this purpose a pendulum clock in which the variation of rate due to the raising or lowering of the centre of gravity of the pendulum under variations of temperature is, through its influence upon the daily error of time shown on the dial, employed for determining the mean temperature of the air throughout the twenty-four hours. And the notice concludes by saying that M. von Sterneck has also proposed to apply the same principle to determine the variations in atmospheric pressure and in the intensity of magnetism.

In reference to this subject it is only right to point out to the readers of NATURE (a term synonymous with the general body of lovers of science all over the world) that the merit of these suggestions is, by priority, due to one of our own countrymen, Mr. W. F. Stanley, who, at the *soirée* given by the President of the Royal Society as far back as April, 1876, exhibited two instruments in which the chronometrical method of determining thermometric and barometric averages was carried out with very marked success.

One of these to which the name chrono-thermometer was applied, consisted of a clock, the pendulum of which was a mercurial thermometer, its centre of gravity being raised or lowered by the expansion or contraction of a column of mercury under variations of temperature. In the other instrument, or chrono-barometer, the pendulum consisted of a glass tube con-

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[1] They are, I find, contained in a paper of some length in vol. vi. of "Taylor's Scientific Memoirs," 1853, pp. 114—162.

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